PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Patient safety initiatives in obstetrics: A Rapid Review
AUTHORS	Antony, Jesmin; Zarin, Wasifa; Pham, Ba; Nincic, Vera; Cardoso, Roberta; Ivory, John; Ghassemi, Marco; Barber, S; Straus, Sharon; Tricco, Andre

VERSION 1 – REVIEW

REVIEWER	Tim Colbourn Senior Lecturer in Global Health Epidemiology and Evaluation, UCL Institute for Global Health, London, UK
REVIEW RETURNED	22-Dec-2017

GENERAL COMMENTS	The authors should be congratulated on a well-conducted and well-written review. I have one important comment about the inclusion criteria, and a few more minor comments. 1. Inclusion criteria: why not extend eligibility criteria from publication in 2015 to 2017? and why only include RCTs? especially given that you didn't find any related to litigation claims (a key aim of your review). If you also include observational studies you may find more information to inform your conclusions. 2. Results, lines 334-336, in reference to Colbourn et al (ref 22): "The RCT was assessed as having a low risk of bias on all items except selective outcome reporting, which was unclear." – please clarify this statement: which outcomes may have been selectively reported? and according to what source? If it is unclear, what makes you suspect selective outcome reporting? same goes for the Lumley trial, line 349. 3. Discussion, lines 385-386: "In addition, a cost-effectiveness analysis could be conducted to determine the cost-effectiveness of these patient safety interventions." – this has been done to some extent, please see pages 293-294 and Table 15.2 of this chapter from DCP3 on RMNCH: http://dcp-3.org/sites/default/files/chapters/DCP3%20RMNCH%20Ch15.pdf 4. Discussion, lines 386-387: "Such a systematic review can include a meta-analysis of the QI strategies versus usual care" (and line 432 conclusion) – would such a future systematic review not run into the same problem as your review in terms of interventions being too heterogeneous to do meta-analyses?

REVIEWER	Nathan C. Sundgren Baylor College of Medicine, Houston, Texas, United States of America
REVIEW RETURNED	12-Jan-2018

GENERAL COMMENTS	The paper is a rapid review of the literature specifically limited in scope to allow a rapid gathering of information for decision makers. The team reviewed RCTs of complex QI interventions at improving maternal and neonatal patient outcomes. Other outcomes of litigation claims and costs of litigation were not able to be assessed for lack of articles discovered in this review process. The paper is generally well-written and it answers the question of the authors' objectives. The methods found in the supplementary material are very helpful and the paper explains the search strategy. The results are difficult to follow. It is difficult to make any meaningful conclusions from the limited number of RCTs studied. The interventions are diverse and no meta analysis can be done. The authors are forthright about this limitation. Table 1 is difficult to follow. Its layout has small print and there are many "other outcomes" listed for which most of the papers did not report leaving a "-". The table is hard to read. There is also clearly an error in the table. The last 2 studies in the columns listed have the exact same sample size as the 3rd and 4th studies listed do, respectively. And these sample sizes are different than those listed in their own supplementary file information and in the results section. The table also has a minor error in formatting, the line between column 4 and 5 in the "Key outcomes" is offset from the others. These errors must be corrected in the table. The discussion does a good job making limited and appropriate conclusions from the review.

REVIEWER	Shigeki Matsubara Jichi Medical University, Japan
REVIEW RETURNED	18-Jan-2018

GENERAL COMMENTS	To authors, The theme dealt here is clinically and socially important. The paper is well written. The heterogeneity of the study designs and also the study populations made meta-analysis difficult and, thus, the authors employed narrative review, which is agreeable. Although important question whether quality improvement intervention reduced the litigation claims and related costs remained unanswered; however, the authors should not be blamed for it. There are some limitations in this study but this is inherent to this type of study and the authors clearly described them. UK English and USA English are mixed; however, I believe that this can be easily revised at the editorial stage.

VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1
Reviewer Name: Tim Colbourn
Institution and Country: Senior Lecturer in Global Health Epidemiology and Evaluation, UCL Institute for Global Health, London, UK
Please state any competing interests or state ‘None declared’: None declared (except that I am the lead author of reference 22, one of the RCTs included in the review)

Comment 1a: The authors should be congratulated on a well-conducted and well-written review. I have one important comment about the inclusion criteria, and a few more minor comments.

Response: Thank you for taking the time to review our manuscript and for providing us with this excellent feedback.

Comment 1b: Inclusion criteria: why not extend eligibility criteria from publication in 2015 to 2017? and why only include RCTs? especially given that you didn't find any related to litigation claims (a key aim of your review). If you also include observational studies you may find more information to inform your conclusions.

Response: Thank you for your thoughtful questions. As this was a rapid review with a 6-week timeline, it was important that we developed inclusion criteria that were both acceptable to the decision-makers and feasible within this time frame. Therefore, we had to methodologically tailor our review to their needs by limiting our search results to RCTs published in English within a 10 year span. In addition, the literature search was conducted in August 2015 and the review as completed on October 2015 for the purpose of submitting a report to the review commissioners, who did not request that we update our findings. In the manuscript, we report the reasoning for our streamlined inclusion criteria upfront (lines 99-108) and discuss potential limitations to consider as a result (lines 423-428).

Lines 99-108: Due to an exponential increase in litigation claims related to patient safety in obstetrical care in South Africa, the World Health Organization (WHO) South Africa – Country Office commissioned a review of patient safety initiatives. In order to provide decision-makers with timely results, a rapid review approach was collectively agreed upon with a 6-week timeline for completion. Rapid reviews tailor the systematic review process to produce information that is relevant to decision-maker needs in an abbreviated period of time.13 The streamlined steps followed in this review included limiting: the study design to randomised clinical trials (RCTs), search dates to a period of 10 years, and language of publication to English.

Lines 423-428: However, as with any rapid review, there are also some limitations to be considered. We had to methodologically tailor our review to suit the decision-makers needs by limiting results to RCTs published in English within a 10 year time frame. In addition, the literature search was conducted in August 2015 for the purpose of submitting a report to the review commissioners who did not request that we update our findings.

Comment 1c: Results, lines 334-336, in reference to Colbourn et al (ref 22): "The RCT was assessed as having a low risk of bias on all items except selective outcome reporting, which was unclear." – please clarify this statement: which outcomes may have been selectively reported? and according to what source? If it is unclear, what makes you suspect selective outcome reporting? same goes for the Lumley trial, line 349.

Response: Using the Cochrane Risk of Bias tool, we assessed a study as being at a high risk of selective outcome reporting bias if all of the study's pre-specified primary outcomes were not reported, or one or more reported primary outcome(s) were not pre-specified (e.g. in the protocol). On the other hand, low risk of bias was assigned if the study protocol was available and all of the study's pre-specified outcomes of interest were reported in the review, or the study protocol was not available but it was clear (e.g. author statement in paper) that the published reports include all expected outcomes, including those that were pre-specified. Because of the absence of a study protocol and a statement about planned versus reported outcomes in the papers by Coulbourn et al. (2013) and Lumley et al. (2006), there was insufficient information to make a judgment either way and therefore these studies were scored as 'unclear' for this component.

Comment 1d: Discussion, lines 385-386: “In addition, a cost-effectiveness analysis could be conducted to determine the cost-effectiveness of these patient safety interventions.” – this has been done to some extent, please see pages 293-294 and Table 15.2 of this chapter from DCP3 on RMNCH: http://dcp-3.org/sites/default/files/chapters/DCP3%20RMNCH%20Ch15.pdf

Response: Thank you for bringing this to our attention. We have removed this line in the manuscript and have referenced the above-mentioned cost-effectiveness analysis in the discussion (lines 442-453).

Lines 442-453: Finally, we did not identify any randomised controlled trials specifically addressing litigation claims or undue costs to the healthcare system. However, evidence from non-randomised studies suggests that there may be a relationship between a reduction in adverse safety outcomes and a reduction in litigation and losses due to medical errors and malpractice. These reports5 31 have found that the introduction of patient safety programs, involving a combination of strategies targeting health systems and healthcare providers, have resulted in the reduction of not only obstetrical adverse events, but also the number of litigation claims and resulting costs. In addition, the community and facility-based interventions evaluated in the Colbourn et al22 trial were shown to be highly cost-effective in an economic evaluation conducted by the study authors.32 Further research is needed to examine the effectiveness and cost-effectiveness of patient safety interventions for adverse events, litigation claims and associated costs.

Comment 1e: Discussion, lines 386-387: “Such a systematic review can include a meta-analysis of the QI strategies versus usual care” (and line 432 conclusion) – would such a future systematic review not run into the same problem as your review in terms of interventions being too heterogeneous to do meta-analyses?

Response: Thank you for your question. In our experience, a systematic review (as opposed to a rapid review) is able to accommodate broader inclusion criteria (e.g. more study designs and outcomes) and therefore include a larger number of studies. With the increase in number of included studies, a meta-analysis of like interventions/populations may be possible. However, we agree that the issue of heterogeneity may still exist and have revised the sentence accordingly (lines 407-410).

Lines 407-410: Such a systematic review may be able to include more studies, allowing the conduct of a meta-analysis of the QI strategies versus usual care and potentially quantifying the effectiveness of these interventions.

Reviewer: 2
Reviewer Name: Nathan C. Sundgren
Institution and Country: Baylor College of Medicine, Houston, Texas, United States of America
Please state any competing interests or state ‘None declared’: None declared

Comment 2a: The paper is a rapid review of the literature specifically limited in scope to allow a rapid gathering of information for decision makers. The team reviewed RCTs of complex QI interventions at improving maternal and neonatal patient outcomes. Other outcomes of litigation claims and costs of litigation were not able to be assessed for lack of articles discovered in this review process. The paper is generally well-written and it answers the question of the authors' objectives. The methods found in the supplementary material are very helpful and the paper explains the search strategy.

Response: Thank you for taking the time to review our manuscript and for providing us with this excellent feedback.

Comment 2b: The results are difficult to follow. It is difficult to make any meaningful conclusions from the limited number of RCTs studied. The interventions are diverse and no meta-analysis can be done. The authors are forthright about this limitation.

Response: Thank you for your comment. As you acknowledge, no meta-analysis was conducted due to the diversity of the interventions, populations and settings in the included studies. Rather, we described the results of each study narratively (lines 245-388) and compiled a comparative summary of study results in Table 1 (pages 13-15). We agree it is difficult to draw definitive conclusions given the number of studies we have included, and as such in the manuscript conclusion we recommend a systematic review (and if possible a meta-analysis) be conducted in the future (lines 460-461). We believe our review provides some insight into the complexity of the topic being studied and highlights some of the variables to be considered when developing multifaceted quality improvement strategies for patient safety.

Lines 245-253 (as an example of one study): Althabe et al19 compared the use of a mandatory second opinion by a clinician trained to use a new decision-aid tool to usual care before caesarean section. This decision-aid tool provided clinicians with suggestions and recommendations on how to prevent non-emergency caesarean sections. This cluster-RCT of 149,276 pregnant women found a small significant reduction in the rate of caesarean section for the intervention versus usual care (relative rate reduction 7.3%, 95% CI 0.2-14.5). Other safety outcomes of maternal, perinatal and neonatal mortality, as well as unplanned admission to the neonatal intensive care (NICU) and intensive care unit (ICU) showed no significant differences between groups. This RCT had an unclear risk of selective reporting bias and other bias.

Lines 460-461: A future systematic review, including a meta-analysis, may be able to provide more definitive conclusions.

Comment 2c: Table 1 is difficult to follow. Its layout has small print and there are many "other outcomes" listed for which most of the papers did not report leaving a "-". The table is hard to read. There is also clearly an error in the table. The last 2 studies in the columns listed have the exact same sample size as the 3rd and 4th studies listed do, respectively. And these sample sizes are different than those listed in their own supplementary file information and in the results section. The table also has a minor error in formatting, the line between column 4 and 5 in the "Key outcomes" is offset from the others. These errors must be corrected in the table.

Response: Thank you for pointing out these inconsistencies. We have revised Table 1 (pages 13-15) by fixing the errors in the sample size row, reducing the size of the “-“ and increasing the sizes of the checkmarks, revising the legend, and reformatting the columns so that they are all aligned. We have also verified that the results in the table, manuscript and supplementary file are consistent.

Comment 2d: The discussion does a good job making limited and appropriate conclusions from the review.

Response: Thank you.

Reviewer: 3
Reviewer Name: Shigeki Matsubara
Institution and Country: Jichi Medical University, Japan
Please state any competing interests or state ‘None declared’: None.

Comment 3a: To authors, the theme dealt here is clinically and socially important. The paper is well written. The heterogeneity of the study designs and also the study populations made meta-analysis

difficult and, thus, the authors employed narrative review, which is agreeable. Although important question whether quality improvement intervention reduced the litigation claims and related costs remained unanswered; however, the authors should not be blamed for it. There are some limitations in this study but this is inherent to this type of study and the authors clearly described them. UK English and USA English are mixed; however, I believe that this can be easily revised at the editorial stage.

Response: Thank you for taking the time to review our manuscript and for providing us with this excellent feedback. We have reviewed the manuscript and attempted to apply UK English guidelines throughout.

VERSION 2 – REVIEW

REVIEWER	Nathan C Sundgren Baylor College of Medicine, United States of America
REVIEW RETURNED	19-Mar-2018

GENERAL COMMENTS	The changes to Table 1 meet my biggest concern.